LETTERS TO THE EDITOR.

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The Present-day Occurrence of Spontaneous Generation.

As is well known, Dr. Charlton Bastian has for several decades been investigating the occurrence of spontaneous generation—the development of living organisms from non-living matter. However opposed to what seems to be our common experience this may be, its occurrence at some time or other is at least suggested by modern doctrines of evolution. Dr. Bastian makes use of solutions containing colloidal matter, from which, if it has done so, living matter may be presumed most probably to have sprung. Although such solutions apparently contain no carbon and other constituent elements of protoplasm, an ample sufficiency of such elements to supply all requirements is present as "impurities" in the solutions.

I have carried out some experiments similar to those of Dr. Bastian, and although I have not yet been able in my laboratory to confirm Dr. Bastian's observations, I have obtained no evidence to prove that his are erroneous. I have on several occasions prepared the solutions, sealed them up in tubes, and submitted these to a *single* boiling. Before sterilisation, subcultures from the solutions have yielded abundant growths of micro-organisms, but after the single boiling I have never obtained growths on subculturing. This suggests that the *three* boilings to which the tubes are commonly subjected does kill all organisms present in them. I have, moreover, never once obtained the common forms of sporing bacilli from the sterilised tubes· had sterilisation been incomplete, these organisms would certainly have been expected.

The structures resembling organisms seen on many occasions in Dr. Bastian's tubes are, I am convinced, really organisms, and are not artefacts, pseudo-organisms, &c.; whether they be living or no cannot, of course, be determined microscopically, unless they be motile, which has been the case on two or three occasions (bacteria). Dr. Bastian has drawn up the following statement in order that his latest results may be brought before the scientific world.

R. T. HEWLETT.

Experimental Data in Evidence of the Present-day Occurrence of Spontaneous Generation.

IN the autumn of 1905 I found that microbes would grow and slowly multiply when inoculated into a weak solution of neutral ammonic tartrate in distilled water, and that though the organisms would grow in this solution without the aid of light, that light distinctly favoured the process, since when an inoculated solution was equally divided, the half which was left exposed to ordinary diffuse daylight became turbid much more quickly than the other half which had been placed within a dark incubator, even though the temperature of this latter was as much as 20° F. higher than that of the portion exposed to daylight.[1]

This was an experience in opposition with previous bacteriological doctrine, and it has been found to be of much importance in connection with experiments which I soon after commenced, and have ever since been continuing, bearing upon the question of the origin of life.[2]

Nature of the Experimental Solutions.

My first experiments were made with ordinary commercial sodium silicate (water-glass) diluted with an equal quantity of distilled water: a few drops of this fluid, varying from 1–8, being added to an ounce of distilled water containing six drops of dilute phosphoric acid and six grains of ammonium phosphate, or else to an ounce of distilled water containing simply eight drops of liq. ferri pernitratis of the British Pharmacopœia.

These solutions at first, and up to the summer of 1910, were the two experimental fluids always made use of, varying only in the number of drops of the dilute sodium silicate employed, in accordance with varying strengths of different samples of this product.

These solutions of water-glass have been found to deteriorate and undergo some slow changes (a rather copious white deposit gradually forms in the bottle in which they are kept), and after about eighteen months my solutions would no longer yield the same kind of experimental results as at first. Moreover, during the last twelve months I have been unable to obtain any satisfactory sample of water-glass.[3]

Strangely enough, Kahlbaum's 10 per cent. solution of sodium silicate, which is a comparatively uniform product, has never yielded any satisfactory results when it has been used in the preparation of my solutions.

On mentioning these troubles in the summer of 1910 to Dr. Otto Rosenheim, of King's College, he kindly gave me some of a very dilute solution of colloidal silica, prepared with great care after Graham's method, and made with the aid of the 10 per cent. solution of sodium silicate above referred to, the use of which had hitherto always proved unproductive.

This solution of colloidal silica gave more uniformly good results than I had ever obtained before, when I used ten to twelve drops of it to the ounce with the usual quantities of dilute phosphoric acid and of ammonic phosphate—though I have never been able to obtain a single successful result when using it with pernitrate of iron in the preparation of the yellow solution. Unfortunately, however, the weak solution of silicic acid, like the common water-glass solution, has seemed gradually to deteriorate, and that, too, much more rapidly, though in appearance the solution shows no change. Up to the present I have never been able to repeat successful results with a second solution when it was more than four months old.

Thus, it seems clear that the specimens of water-glass with which I first experimented successfully must have contained other favouring ingredients not present in the 10 per cent. solution of sodium silicate; further, that though this solution yielded only barren results, yet the colloidal silica prepared from it and from strong hydrochloric acid yielded the best results of all when used as an ingredient of one of the colourless solutions, but uniformly poor results when mixed with iron for a yellow solution.

From the point of view of the capability or the reverse of the different fluids for engendering living matter (owing to differences in chemical composition) these seeming contradictions may have no real significance; though the opposite point of view that my positive results may be due to the pre-existence of organisms in these particular solutions and not in the others which yielded negative results is a position that would seem quite impossible of reconcilement with the variations in composition above cited—even if the process of sterilisation had not intervened.

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[1] See *Knowledge*, August, 1905, p. 199.

[2] See "The Evolution of Life," 1907; and "The Origin of Life," 2nd edition, 1913 (Watts and Co.)

[3] Details on this subject will be found in "The Origin of Life," 2nd edit., pp. 86–91. Recently, however, Messrs. Allen and Hanbury have put me in communication with the London agents of their makers of water-glass, who have kindly supplied me with some samples of different specific gravity. An examination of them leads me to believe that the recent failures have been due to my having been supplied with samples of higher alkalinity than formerly. I have made new trials with a sample of 75° sp. gr., after diluting it with an equal bulk of distilled water and its behaviour with the other reagents (in their usual proportions) seems to be similar to that of the samples which I obtained in 1910, so long as *four* or *five drops* to the ounce are used for the yellow solution, and *three drops* to the ounce for the colourless solution. Samples of this diluted solution may be obtained from Messrs. Allen and Hanbury, of 6 Vere Street, W.

Sterilisation and After-Treatment of the Experimental Vessels.

The experimental tubes were prepared in this manner. A little more than half an ounce of either of the solutions was put into each of a number of sterilised glass tubes. These were then hermetically sealed, and subsequently heated for five to twenty minutes to temperatures ranging from 125° to 145° C., or else to 100° C. for twenty minutes on three successive days.

As in all previous experiments concerning the possibility of spontaneous generation by Pasteur, Pouchet, Tyndall, and many others, the destructive influence of heat was relied upon for ridding the fluids and vessels of any pre-existing living things that might be contained therein—these being very much less numerous in my saline solutions than in hay infusions and other organic media of which it was the custom formerly to make use.

Saline solutions were used by me because they could be submitted, within limits, to higher temperatures than organic infusions without destroying any possible productivity; and because in using them there would be a closer approximation to the conditions that must have existed when the surface of our earth first cooled down below the temperature of boiling water, so that a natural origin of living matter might thereafter become possible.

After sterilisation the sealed experimental vessels are exposed to diffuse light and a varying amount of actual sunshine for periods of from four to ten months or more before the contents of the tubes are examined microscopically, though the terminal month may, with advantage, be passed in an incubator at some temperature between 27° and 37° C.

When "controls" are opened, say any time within one or two weeks of sterilisation, no organisms, except it may be one or two embryonic forms, are to be found, especially if the solution of ammonic tartrate has been filtered through No. 0 Swedish paper, though when other tubes of the same series come to be examined after the several months of exposure to light and heat above mentioned many well-developed organisms are often found, which can be proved to be living.

These organisms are for the most part Torulæ, and minute simple moulds of different kinds. Specimens of such organisms are shown in Fig. 1, as they were taken direct from the tube, the Torulæ in this case being unusually abundant.

Bacteria are much less frequently met with, mostly motionless, though occasionally motile. Plasmogenic products simulating cocci and bacilli in appearance are also by no means uncommon in these colloidal silicate solutions.

According to De Barry and other authorities, no germs of moulds can survive a single immersion for a few minutes in water at 100° C.; while Torulæ are uniformly admitted to be killed by immersion for a minute or two in water at 60° C.

I have ascertained that the mixed Torulæ and fungus-germs to be found in the bloom on the surface of grapes have been killed by immersing the grapes for only *thirty seconds in boiling water*.[4]

Further, I have found that the Torulæ and minute moulds that tend to appear after a short time in unheated weak solutions of silicic acid are, like other fungus-germs, unable to survive a single boiling for five minutes. Yet the least severe sterilising heat employed in my experiments has been a boiling for twenty minutes on three successive days.

Objections and Replies Thereto.

Those who rely upon existing evidence as to the thermal death-point of such organisms as have been found within my tubes (rather than like Sir E. A. Schäfer pinning their faith to mere preconceptions as to the impossibility of the origin of living matter in these particular solutions) will agree that the sterilising processes employed by me should have been very much more than adequate to kill any germs of Torulæ or moulds that may have pre-existed within the tubes.

Those who are incredulous as to my results are compelled, therefore, to fall back upon one or other of the three following objections:—

(1) There are first the mere surmises of superficial objectors who postulate contaminated pipettes, or the dropping of organisms on to the microscope slide from the atmosphere before the application of the cover-glass. These are puerile objections against a prolonged research such as mine. Of course, pipettes have been carefully sterilised immediately before use; and as for the dropping of organisms from the atmosphere, such objectors would find it hard enough, if they tried, to find definite kinds of organisms, and often numbers of them (as in Fig. 1) on slides and under cover-glasses prepared by themselves. The next paragraph, however, will show the unreality of these mere surmises.

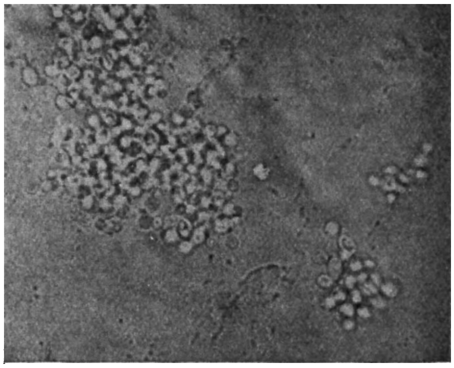

FIG. 1.—Large Group of Torulæ as taken direct from tube. No. 289. ×500.

(2) It is assumed by many that the bodies found by me in my tubes are not really organisms. It is thought that they must be mere plasmogenic simulacra of living things, such as Leduc, Herrera, the brothers Mary, and many others have found in silicate solutions. This objection has been made over and over again. It is true that such bodies are occasionally to be met with in my solutions, and however important these mere simulacra may be as intermediate products between living and non-living matter, the other bodies which I find are not of this order. Those who have seen some of my tubes opened, and the bodies in question taken therefrom, such as Profs. Hewlett and Shattock, Profs. Farmer and Blackman at the College of Science, and several others, are unable to doubt that they have seen actual organisms taken from the tubes.

Moreover, on August 18 I received a letter from Paris written by two celebrated plasmogenists, Albert and Alexandre Mary, in which they told me that they had confirmed my experiments. Following my directions implicity, they had, after some months, found typical Torulæ and Micrococci within the tubes, and had convinced themselves that they were actual living organisms. Thus, in regard to the latter, they say: "les ayant inoculés dans des solutions de glucose à 2 per cent. avec une légère quantité de lactate de fer, les microcoques plus haut décrits ont proliféré d'un façon remarquable, et la culture a offert l'aspect d'un sédiment se réunissant au fond des tubes."

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[4] See "The "Origin of Life," 2nd edition, p. 96 (Watts and Co.)

This adhesion to my views by Albert and Alexandre Mary should be a complete answer to the second objection, so often formulated, that the bodies found by me were only plasmogenic products such as Leduc, Herrera, they themselves, and others had previously described as occurring in colloidal solutions, and should go far towards meeting the final doubt—the only one open to those who in this country have seen what they believed to be actual organisms taken from my tubes, namely the doubt whether the organisms, which they were bound to recognise as such, were still living.

(3) This brings me to the final objection advanced by some. They admit that many at least of the bodies that have been photographed are organisms, but believe them to be merely organisms that pre-existed in the solutions, and which, when found, were dead, having been killed by the sterilising process to which the tubes had been submitted.

As to this, it must never be forgotten that minute organisms are either very scarce or not to be found at all in "control" tubes opened soon after sterilisation, and to be often abundant after months in other tubes of the same series which have been exposed to light and heat and which had never previously been opened. If they were not there at first, and are there in numbers subsequently, how are we to resist the conclusion that they are living and that *they have developed and multiplied within the previously sterilised tubes?*

In illustration of this important point I may state that I have recently received from New York two slides containing swarms of stained bacteria. These were taken by Dr. Jonathan Wright, the director of the Post-graduate Laboratories there, and his principal bacteriologist, Dr. MacNeal, from tubes which they had prepared and sterilised. They had been repeating my experiments, at first with negative results —even though three of their tubes had been inoculated with a culture of the hay bacillus previous to the triple heating. The organisms on the slides sent to me had been taken from tubes of two other series—one of them sterilised fifteen months, and the other four and a half months previously. The experimenters had some doubts at first whether the very numerous bodies on the first slide were really bacteria, though no such doubt was entertained by Dr. Hewlett or myself. In the second case they reported that they had found what were unquestionably bacteria in "enormous numbers." They now, at first, inclined to the belief that notwithstanding their enormous numbers the bacteria found must have been "in the original materials." But in the last letter received from Dr. Wright he reported that they had made a bacteriological examination of the materials in question with negative results. He adds: "So far as we have gone, therefore, we cannot take refuge in the supposition either that these organisms are crystalline simulacra of life, or that they were derived from the original materials, and were killed but not disintegrated by the triple heating. We have no suggestion to make other than your interpretation, and indeed we desire to be entirely non-committal as yet." I am, therefore, waiting for information concerning the examination of other tubes of these two series.

These facts would seem sufficiently to answer the third objection now under consideration. Still, one very remarkable example of this kind ought to be cited. A series of five tubes containing sodium silicate and pernitrate of iron was boiled for twenty minutes on May 17, 18, and 19, 1912, and these tubes were exposed to light and heat in the usual way. At the expiration of seven and a half months (December 9, 1912) I opened one of these tubes, and took from it a small amount of reddish sediment, similar to that which existed in each of the others. On microscopical examination I found in this sediment two minute masses of mould associated with compound spore-like bodies such as I had never seen before. I sent the specimen to an eminent authority, Mr. Geo. Massee, of Kew, and was told that the mould with its peculiar spores was allied to the genus Oospora. At the end of February of last year another of these tubes was opened by Profs. Hewlett and Shattock; early in March one was opened by me in the presence of Profs. J. B. Farmer and V. H. Blackman; and in May another was opened by me in the presence of some bacteriologists and chemists at the Lister Institute, and in each case more or less of the characteristic Oospora spores were found. The mycelium was not in each case found, and I know that some of the observers were sceptical as to the nature of the spores.

The last of these tubes was kept by me for some future occasion, and was not again particularly noticed until July 22. Then, on examination of the unopened tube, much to my surprise there was to be seen at the bottom, by the side of the sediment, two tufts which had all the appearance of being moulds, one of them about half an inch in diameter and the other smaller. These were seen by many others, in the unopened tube, who took the same view as to their nature. On October 3 this tube was opened by Prof. Hewlett in his laboratory, and he took therefrom, as I expected, some of the Oospora. Portions subsequently taken by me were photographed, and one of them is shown in Fig. 2. That this mould had grown within the sterilised tube is perfectly clear; yet several of those already mentioned had failed in their various efforts to obtain cultures from samples that were found in the other tubes.

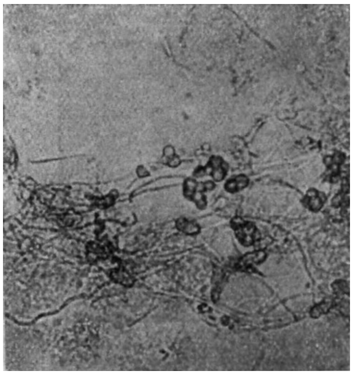

Fig. 2.—Portion of a large tuft of mould (Oospora) which was seen growing at the bottom of tube No. 358. ×325.

Successful cultures of organisms obtained from the tubes may occasionally be obtained by inoculating some of the organisms found into sterilised 3 per cent. glucose or ammonic tartrate solutions. Torulæ will often multiply or moulds will develop as a result, after several days, in such solutions. Fig. 3 shows a number of Torulæ which had thus multiplied within a glucose solution after six days.

Another and a more ready means of proving that the organisms taken from the tubes are living has been commonly adopted by me. The cover-glass of the microscope slip on which they are contained, is at once ringed with paraffin melting at 40° C., and the slip is then put aside in a warm place for a few days. Fig. 4 shows a portion of a mould that had developed, and Fig. 5 shows Torulæ that after several days had greatly multiplied, under such conditions. Why the taking of the organisms out of the tube, and placing them between two layers of glass surrounded by paraffin should so greatly favour their development I am unable to say, but that it does do so I am perfectly certain. I will cite one very striking instance that I observed a short time since. In some of the sediment taken from the centrifuged contents of a tube, I found, beneath the cover-glass, during a thorough examination, about forty to fifty minute solitary bodies like embryo Torulæ. The cover-glass was ringed and the slide put aside. When I examined it again after only thirty hours, in place of the solitary bodies groups were seen of larger bodies from which hyphæ were being developed in almost all cases. I have several photographs illustrating this, and one of the largest of the groups is shown in Fig. 6, while another group is shown in Fig. 7, as seen some days later, under a lower magnification, but in which the hyphæ had grown considerably longer. The tube had been prepared and sterilised many weeks previously, and during that time within the tube only very minute solitary bodies had been produced. But in thirty hours after having been taken out of the tube and placed beneath the cover-glass they grew, they multiplied, and developed hyphæ, as shown by the photographs.

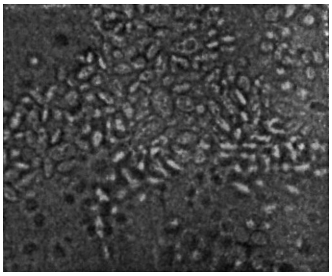
FIG. 3.—Torulæ from 3 per cent. glucose on sixth day after its inoculation from tube No. 437. ×500.

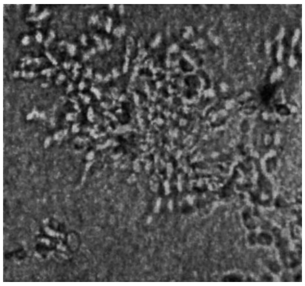
FIG. 4.—Mould from 3 per cent. glucose on ninth day after its inoculation from tube No. 557. ×500.

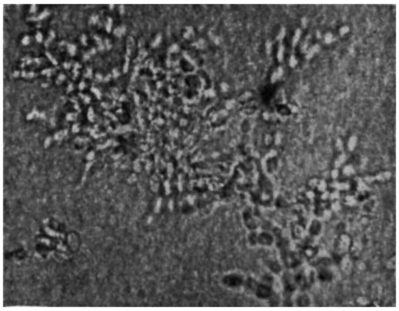
FIG. 5.—Large mass of Torulæ as found beneath a ringed cover-glass on fourteenth day, from tube No. 438. ×500.

From the evidence above detailed it seems very difficult to resist the following conclusions:—

(1) That the bodies alleged to have been taken from the experimental tubes have really been taken therefrom, and are not mere accidental products which have dropped from the atmosphere during the transit of the sterilised pipette from the tube to the microscope slide.

(2) That the bodies in question are actual organisms, and not mere plasmogenic simulacra of living things, such as are often to be found in colloidal solutions.

(3) That they are actual living organisms which, as shown by the evidence of the "control" tubes, have increased and multiplied within the tubes, and will often behave in a similar manner after they have been taken from the tubes and placed under favourable conditions.

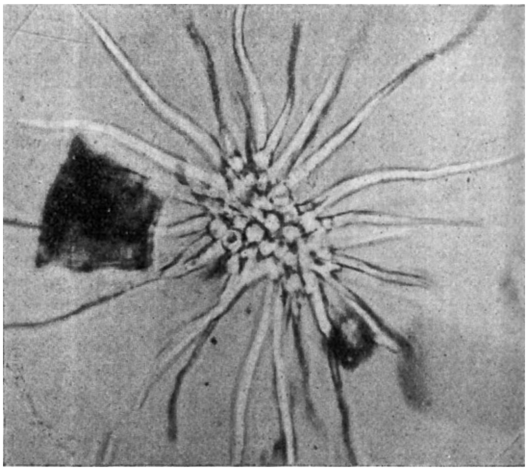
FIG. 6.—Multiplication and development of fungus-germs from tube No. 631, after thirty hours under ringed cover-glass. ×500.

(4) That as all the organisms in question have been shown to be killed by a brief single exposure in fluids to the temperature of boiling water (100° C.), none of them, even if present, could have survived the much higher or much more prolonged heatings to which the tubes and their contents were exposed during the process of sterilisation—that is to say, these tubes should have been after that process as devoid of living things as was our earth in the far remote past, and just as then, at some period, there must have been, as is now generally admitted in the world of science, a *de novo* origin of living matter on the previously lifeless earth under the influence of purely natural causes, so now it would seem that the simple living organisms which appear within the experimental tubes must have been produced, *de novo*,

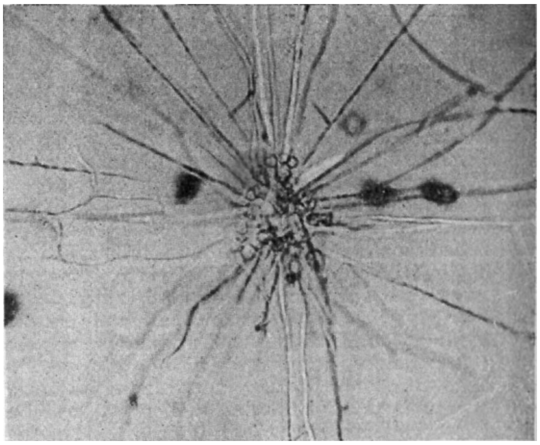
FIG. 7.—Further development of another group of fungus-germs under the same cover-glass, as seen six days later. ×325.

under the influence of physico-chemical processes of the same order as those that must have been operative in the past.

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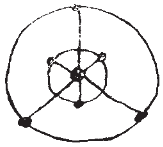
In my work entitled "The Nature and Origin of Living Matter," an abridgment of which has been published by the Rationalist Press Association,[5] I have considered the question of "spontaneous generation" from a broader point of view (pp. 128–141), and have endeavoured to show how multitudes of facts can be explained in accordance with my views, that from the time when living matter first appeared upon the earth it has probably ever been constantly re-appearing, as at present, and giving birth to the simplest living units, such as now swarm upon its surface. These processes are such that they must always take place beyond our ken, seeing that they necessarily begin with mere molecular collocations, gradually going on to the formation of particles of an ultra-microscopic order. Such infinitesimal particles gradually emerge into the region of visible things as revealed by high powers of the microscope, and take on this or that simple organic form in a manner that (though by processes much more complicated) is somewhat akin to the mode by which crystals emerge from different mother liquors, and take on this or that particular crystalline form.

Thus, while the fact of the present occurrence of the *de novo* origin of living matter is, in my opinion, beyond the region of doubt, I fully recognise that the actual steps of the process remain to be discovered. I have elsewhere[6] referred to some of the probable steps of the process, and the prominent part that may be taken by inorganic catalysers under the influence of sunlight, and in some quite recent experiments by Prof. Benjamin Moore and J. A. Webster, in a paper entitled "Synthesis by Sunlight in Relationship to the Origin of Life,"[7] they have been able to demonstrate the probable actual first step of such a process—one that is known to occur as a first step in the nutrition of plants. We are, however, as yet only on the threshold of anything like an explanation of the various stages of this supreme mystery, for the unravelling of which philosophers and chemists have hitherto striven in vain. As with many other natural phenomena, the fact of the occurrence of which cannot be questioned, so here only conjectures are available as to the precise mode in which it may have been brought about. We must, however, repose our faith in the uniformity of natural phenomena, as one of the cardinal postulates of science, and if living matter had a natural origin in the far-distant past, there is, from that point of view, good ground for believing what our experiments seem to testify, that it also occurs at the present day.

H. CHARLTON BASTIAN.

Atomic Models and X-Ray Spectra.

IT is universally assumed that the atom of an element can form a Saturnian system with more than one ring of rotating electrons, and this idea is used in particular by Moseley in the theoretical discussion of his recent experiments. But in an Adams prize essay, not yet published *in extenso*, this is shown to be impossible. If the law of repulsion between two electrons, or of attraction between electron and nucleus, is that of the inverse square, more than one *coplanar* ring cannot exist. All the electrons in any plane must lie in the same ring, and even if they are in different planes, the radii of the rings must be nearly equal. A consideration of a simple case will illustrate this. For example, it is at first sight probable that the system in the accompanying diagram, consisting of two coplanar rings of three electrons each, symmetrically arranged with all the angles equal to 60°, can exist with some angular velocity ω, if the radii of the outer and inner rings are a and b, the latter being much smaller than a. But it is easily shown that the conditions of steady rotation of such a system are—



$$\frac{m\omega^2}{e^2}=\left(\mathrm{N}-\frac{1}{\sqrt{3}}\right)\frac{1}{a^3}-\frac{1}{a(a+b)^2}+\frac{b-2a}{a}\cdot\frac{1}{(a^2+b^2-ab)^{\frac{3}{2}}}$$
$$=\left(\mathrm{N}-\frac{1}{\sqrt{3}}\right)\frac{1}{b^3}-\frac{1}{b(a+b)^2}\quad\frac{2b-a}{b}\cdot\frac{1}{(a^2+b^2-ab)^{\frac{3}{2}}}$$

and the resulting equation for the ratio b/a has only *one* root $b/a=1$, whatever value be attached to N, where Ne is the charge on the nucleus. Any other simple case which is tried will be found to lead to the same conclusion.

This conclusion not only belongs to any ordinary dynamical theory of the rings, but to Bohr's theory also. For Bohr supposes that the *steady rotation* of the system can be derived by ordinary mechanics, and, in fact, the equation so derived is vital to his formula for spectra. If Bohr's theory is to remain—and it is so attractive that its retention is desirable, in the writer's opinion—we must give up the idea of concentric rings in the atom, with X-radiation coming from an inner ring. For any way of avoiding the present conclusion, for example, by making a change in the law of attraction in the immediate neighbourhood of the nucleus, at once destroys the formula on which Moseley bases his view that his experiments support Bohr's theory.

How then, if there is only one ring of electrons, and if X-radiation is due to a ring—a point on which Moseley has given a cogent reason for doubt—does X-radiation originate? The answer is that without a serious reduction in its radius, the single ring may, on Bohr's theory, give radiation of the X-ray type. For Bohr's spectral formula for an atom with nucleus Ne and n electrons is

$$\nu=\frac{2\pi^2me^4}{h^3}(\mathrm{N}-\mathrm{S}_n)^2\left(\frac{1}{\alpha^2}-\frac{1}{\beta^2}\right)$$

where α in the Balmer series is 2, and β takes integral values. But the principal line given by the formula corresponds to $\alpha=1$, $\beta=2$, and its wave-length in cm. is at once found to be

$$\lambda=\frac{1215{\cdot}7\ \ 10^{-8}}{(\mathrm{N}-\mathrm{S}_n)^2}$$

To obtain a wave-length $\lambda=3{\cdot}368.10^{-8}$, Moseley's value for calcium, we only require $\mathrm{N}-\mathrm{S}_n=18{\cdot}98$, which Moseley interprets as meaning $\mathrm{N}=20$, $n=4$. Thus a ring which gives the ordinary hydrogen spectrum when $\mathrm{N}=1$ can give an X-ray spectrum when $\mathrm{N}=20$, in spite of the enormous difference of wave-length concerned. But the *radius* would not be so widely different in the two cases. For in the normal atom it is inversely as $\mathrm{N}-\mathrm{S}_n$, and since on Bohr's view, the radius of a hydrogen atom is $5{\cdot}5.10^{-9}$ cm., that of calcium would be $3{\cdot}10^{-10}$, quite a possible value.

There is ground, accordingly, for retaining Bohr's theory, if only one ring exists, and then the calcium X-ray spectrum means exactly the same thing as the ordinary hydrogen spectrum, and no element should show such X-ray spectra until N becomes large. The X-ray or Balmer spectrum of helium, for example,

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[5] Watts and Co., 1910.

[6] "The Origin of Life," 2nd edition, 1913, pp. 61–65.

[7] Proceedings of the Royal Society, B. 593, p. 163.